Stress—Its Definition.

THE important word *stress*, denoting a fundamental conception in dynamics, is one as to the meaning of which no haziness or doubt ought to be permitted by the scientific community.

In your review of Prof. Gray's "Physics," the reviewer criticises the use made of the word in question, and makes the statement: "Strictly a stress is measured by the force applied per unit of area; it has the dimensions of force divided by the square of a length. . . ."

No authority is quoted to justify this statement. Does such authority exist? On collating the statements regarding the meaning to be attached to the word in some of the most authoritative works in the language, I have found a considerable want of agreement.

Going back to Rankine, who is credited with having introduced the word *stress* as a technical term into mechanics, we find the following paragraph in his paper "On Axes of Elasticity and Crystalline Forms" (1855):

"In this paper the word *Strain* will be used to denote the change of volume and figure constituting the deviation of a molecule of a solid from that condition which it preserves when free from the action of external forces; and the word *Stress* will be used to denote the force, or combination of forces, which such a molecule exerts in tending to recover its free condition, and which, for a state of equilibrium, is equal and opposite to the combination of external forces applied to it."

Again, in his "Applied Mechanics" (1860), we find, in § 86:

"*Stress, its Nature and Intensity.*—The word STRESS has been adopted as a general term to comprehend various forces which are exerted between contiguous bodies, or parts of bodies, and which are distributed over the surface of contact of the masses between which they act. The INTENSITY of a stress is its amount in units of force, divided by the extent of the surface over which it acts, in units of area."

Then, in § 87, Rankine classifies three kinds of stress, (1) *Thrust or Pressure* (2) *Pull or Tension*, and (3) *Shear*, or *Tangential Stress*.

Further, in §§ 96: "*Internal Stress in General.*—If a body be conceived to be divided into two parts by an ideal plane traversing it in any direction, the force exerted between those two parts at the plane of division is an *internal stress*."

Clerk Maxwell, in "Matter and Motion," Art. 37, says: "The mutual action between two portions of matter receives different names according to the aspect under which it is studied, and this aspect depends on the extent of the material system which forms the subject of our attention.

"If we take into account the whole phenomenon of the action between two portions of matter, we call it Stress. This stress, according to the mode in which it acts, may be described as Attraction, Repulsion, Tension, Pressure, Shearing Stress, Torsion, &c."

Again, in Art. 101.—"Stress.

"The next step in the science of force is that in which we pass from the consideration of a force as acting on a body, to that of its being one aspect of that mutual action between two bodies, which is called by Newton Action and Reaction, and which is now more briefly expressed by the single word Stress."

Thomson and Tait's "Natural Philosophy" (1867), Art. 658 (referring to the theory of elastic solids). ". . . the forces called into play through the interior of a solid when brought into a condition of strain. We adopt, from Rankine, the term *stress* to designate such forces, as distinguished from strain defined to express the merely geometrical idea of a change of volume or figure."

Thomson (Kelvin) in the 9th edition of the "Encyclopædia Britannica," article "Elasticity": Mathematical Theory, Chap. i. "Def. A stress is an equilibrating application of force to a body."

Tait, in "Newton's Laws of Motion" (1899), Art. 45:

"A pair of equal and oppositely directed forces, acting in one line, is a particular case of what is now called a *Stress*. The stress along a stiff rod (necessarily the same across every transverse section) may be either a *Thrust* or a *Tension*, that along a string or chain can be a *Tension* only. [But the term stress, in its widest signification, means *any* system of equilibrating forces.]"

"In a fluid the stress at any point is generally what is called *Hydrostatic Pressure*, whose characteristic is that the stress is the same across a small given plane area. . . . In all these cases the stress is measured by the amount per unit area of the surface on which it is exerted."

Love, in "Theoretical Mechanics" (1897), Art. 122, defines the stress at a point A across a plane interface passing through A, as the force per unit area exerted across a small area whose centroid is A.

From the preceding quotations there would seem to be a double ambiguity in the present usage of the word *stress*.

Firstly, it may be used to denote the whole mutual action between two portions of matter, A and B, say, in which case it would be specified by stating the force or system of forces exerted either by A upon B, or by B upon A; or it may be used to denote the *force per unit area* exerted by A upon B. The latter is clearly less widely applicable (torsional stress, *e.g.*, cannot be reckoned per unit area), and corresponds to what Rankine calls *intensity of stress*, or what is by some teachers appropriately named *unital stress*.

Secondly, the term *stress* may be defined as in the "Elasticity" article in the Encyclopædia to be an "equilibrating application of forces," or, as by Maxwell, to be the complete phenomenon including the "Action and Reaction" of Newton's Third Law of Motion.

To my mind there can be no doubt as to the greater usefulness of the latter definition, even though the former may be more consistent with some of Rankine's statements on the subject. It will be noted that in my quotation from Prof. Tait's work there seems to be a vacillation between the two meanings (what is meant there by "stress across a transverse section," or "stress across a small plane area"?), though he explicitly adopts the former alternative; and in the paragraphs of "Thomson and Tait" immediately following that quoted above there seems to be a similar shifting of ground in applying the term, while Maxwell's use of the word is consistent with his clear definition. This in itself argues strongly for the Maxwellian use of the word. Besides, the "equilibrating application" definition would seem to leave us in the lurch when we wish to name the internal forces of bodies not in equilibrium. And all who have had much experience in teaching dynamics to beginners must appreciate the help which the word in its Maxwellian sense affords in getting the student to see the difference between reaction and equilibrant, and to stop asking one such conundrums as "If action and reaction are equal, why does a body move?" And of course it is precisely the beginner for whose benefit we should take the trouble to be consistent in the use of words.

Let me conclude by offering the following suggestions for what they are worth:—

(1) Let the word "stress" be defined and used as in Maxwell's "Matter and Motion."

(2) Let "unital stress" or "unital stress at a point across a plane" be used as defined in §122 of Love's "Theoretical Mechanics."

R. F. MUIRHEAD.

Glasgow, June 4.

I HAVE to thank the Editor for his courtesy in allowing me to see Mr. Muirhead's interesting letter. I quite agree that the meaning attached to the word "stress" by eminent writers during the fifty years from the time of Rankine to the present day has varied. At the same time, I observe that the only two definitions of the "measure of stress" which are quoted are of recent date, and both state clearly that a stress is measured by the force per unit area, though I find this same definition in Thomson and Tait, 1867 edition, Art. 661, a few lines below the quotation given by Mr. Muirhead. I think, then, I may claim sufficient authority for my statement, "Strictly a stress is measured by the force applied per unit of area," and for the doubt which I expressed as to the desirability of introducing the word "stress" as practically synonymous with "force" in a discussion of Newton's second law of motion.

While I share Mr. Muirhead's regret at the limitation thus imposed on the meaning of a general term "stress" as indicating the mutual action between two bodies, I hardly think his suggestion to distinguish between "stress" and "unital stress" will meet the case.

REVIEWER.

Hybrid Oochromy, with a Note on Xenia.

IN a note on "Telegony, Xenia and Hybrid Oology,"[1] which appeared in *Natural Science* (vol. xiv. p. 394, 1899), I introduced the last-mentioned term to denote a singular phenomenon

[1] At the request of the editor I have altered the term hybrid oology to hybrid *oochromy*, which I agree is in many ways better, except that it would seem to refer to the coloration of the egg to the exclusion of its microscopic structure.

said to have been observed in birds, viz., that when a hen is fertilised by a cock of another kind the resulting egg is contained in a shell tinted, more or less, like those laid by the cock's own breed. At the time, I must confess, I was rather inclined to doubt if it did really occur, or if it were not a simple reversion, or a mistake, when my attention was drawn still closer to the subject by a friend who had kindly offered to assist in obtaining, if it were possible, additional proofs of telegony by first crossing a canary hen with a greenfinch cock and then returning her to her own breed. This was done, and resulted in three eggs being laid to the greenfinch. Their shells were all tinted more like the eggshells of a greenfinch than those of a canary. Two of these eggs were afterwards found to be infertile. This showed that the alteration in the tint of the eggshell had nothing to do with the nature of the fertilising spermatozoon. But the occurrence of hybrid oochromy could not be said to have been proved, for there is very little difference in the tinting of the eggshells of a canary and greenfinch, and I do not know whether the canary was purely bred or not.

I was thus anxious to find out for certain whether or not such an occurrence was possible. I therefore obtained three black Minorca hens, which had come of stock that had been purely bred for the last twenty years. The Minorca breed is the oldest variety of the famous Spanish fowls, of which the origin seems older than the recollection of it![1] These three Minorca hens I penned up alone for more than four weeks, during which time thirty-two eggs were laid, and the shells of all of the later ones were of a very pure white colour.

The reason I had kept them alone for so long a time was that I required eggs entirely free from the intervention of any cock, and the commonly accepted opinion of poultry fanciers seemed to be that a period of nearly three weeks was necessary for the complete extermination of spermatozoa. However, to prevent any mistake, at the end of this time three eggs were artificially incubated for a period of forty-eight hours at the Durham College of Science, and they proved quite infertile.

After having thus demonstrated that the Minorca egg is contained in a pure white shell, I introduced into their pen a buff cock of the Cochin China breed, a breed famous for the brown with which its eggshells are tinted. The second egg laid after its arrival in the pen was provided with a shell of a very decided brown tint, and among a dozen or more laid within the succeeding two or three weeks, the shells of several were of a faint brown tint.[2] I was, however, unable to observe any difference in the microscopic structure of the eggs, such as is described by Herr von Nathusius. (See "Dictionary of Birds," by A. Newton, p. 190.)

This remarkable case appears to me to be an almost incontestable proof that hybrid oochromy does, at times, occur, as the only other way for accounting for pure bred black Minorca hens laying brown tinted eggs would be that they were reverting to some brown-egg-laying ancestors, a very unlikely supposition when we remember the age of the breed.

The next question to answer is—How does hybrid oochromy take place? I feel quite convinced, both from my own observations and those with the above-mentioned canary, that the tint of the eggshell is not, and cannot be, affected by the nature of the fertilising spermatozoon, and so we must turn our attention to the spermatic fluid, the chemical properties of which, acting in conjunction with those of the products of the shell-gland, will probably be found to be sufficient to cause this change of tint.

Hybrid oochromy has, in company with a closely associated phenomenon in another kingdom (I refer to Xenia), often been referred to as a case that cannot be explained by the Weismannian theory of heredity, *i.e.* the continuity of the germ-plasm. If the above explanation (and I can suggest no other) of hybrid oochromy should be proved to be correct, it is easily seen to be merely a chemical change and wholly apart from the phenomena of fertilisation. In the same way I should think it is very possible that xenia might be found to be not unconnected with the conjunction of the male and female elements forming the endosperm. It doubtless will be shown before long whether or not these two attempted explanations be correct. They will, I hope, however, tend to lessen the opposition to the Weismannian theory by showing how a fact which, at first sight, appears absolutely antagonistic thereto is found to be in complete accordance with it. It also shows what a deep effect may be induced in living organisms by the interaction of the chemical products of their glands.

I must here take the opportunity of expressing my best thanks to the Durham College of Science, Newcastle-on-Tyne, for allowing me the ground, &c., on which to conduct the experiment.

G. P. BULMAN.

Newcastle-on-Tyne.

[1] "The Poultry Book," by Lewis Wright. Popular edition, p. 340.

[2] Since writing the above I have incubated two of these eggs and found them fertile. At first sight this would seem to contradict the explanation given, but although I hold that fertilisation is not necessary, it certainly may take place in some cases.

The Swimming Instinct.

I HAVE just tested the inherited powers of swimming in newly hatched pheasants. I find that when placed in tepid water, at the age of about thirty hours, they swim easily with well-co-ordinated leg-movements and show very little signs of distress.

C. LLOYD MORGAN.

University College, Bristol, June 24.

RECENT SCIENTIFIC WORK IN HOLLAND.

BEGINNING with that which is of most general importance, we draw attention to the recent work of Prof. Hugo de Vries, of Amsterdam. Prof. de Vries, who is well known as a botanist and biologist and whose name is familiar to those acquainted with the history of modern chemistry, has just published the first part of a book entitled "Die Mutationstheorie. Erster Band. Versuche und Beobachtungen über die Entstehung von Arten im Pflanzenreich" (Leipzig: Veit, 1901), containing, as the title indicates, the account of a series of observations on the formation of new species in plants. Starting from the fact, well known to florists, of the appearance of "single variations" in their flower-beds, de Vries has been trying to find wild flowers which would show the same phenomenon. Of the 100 species investigated only one appeared to possess the property which was looked for, the *Œnothera Lamarckiana*, originally from America, but at present growing wild in Holland. Now about ten years ago de Vries transferred specimens of this plant to the botanical gardens at Amsterdam, and up to date he has studied as many as 50,000 of its descendants.

Of these 50,000 about 49,200 were in no respect different from the original patriarchal *O. Lamarckiana*, showing no tendency towards gradual change in any special direction, but only the common small fluctuating "variations" as regards size and appearance on either side of a normal, in fact resembling in that respect other plants and animals which are left to themselves without being interfered with.

Quite otherwise the 800 other plants. None of these, although appearing spontaneously, could be said to be representatives of the species *Lamarckiana*, from which they were descended. De Vries arranges them in seven distinct species, viz. 1 of *O. gigas*, 56 of *O. albida*, 350 of *O. oblonga*, 32 of *O. rubrinervis*, 158 of *O. nanella*, 221 of *O. lata* and 8 of *O. scintillans*. Now comes the crucial question of the whole investigation. What right has de Vries to look upon the differences between these seven species and the original species as being of a different order from the variations between the specimens of each species, and what entitles him to call these differences *mutations* as opposed to variations? The answer is this: a representative of these new species produces descendants the majority of which unmistakably belong to the same species as itself. Not all the new species behave in the same way; as an instance, the only representative of *O. gigas* was isolated and made to fertilise itself. From it were obtained 450 plants, all of which, with only one exception, were *O. gigas*, the one exception not being a return to *Lamarckiana* but belonging to a new variety. The plant is a strong one and retains its properties in subsequent generations so far as investigated.